the habits of animals are recorded on personal observation they have evidently been accurately and carefully noted. The author is a genuine naturalist with a thorough love and admiration for animals, and in consequence he possesses considerable power of understanding and appreciating them. Secondly, the woodcuts are numerous and for the most part excellent. A few, such as the big-headed Gaur (*Bos gaurus*) on p. 530 and the musk-deer on p. 493 are less successful, and it may be questioned whether a nylgao can stretch itself into the gallop depicted at p. 477, but the spirit of the cut last-named would atone for a worse fault, and there is far more ground for admiration than for criticism. As an amusing work, with good illustrations, to which residents in India may have recourse for the identification of the principal mammals, this volume will probably find a ready place in the Anglo-Indian library. For the determination of the smaller kinds, and for a knowledge of the less known and more difficult species, the student will do well to search elsewhere.

W. T. B.

NORTH AMERICAN MOLLUSCA

A Review of the Non-Marine Fossil Mollusca of North America. By Charles A. White. (Washington: Government Printing Office, 1883.)

THE Hon. J. W. Powell, the Director of the Geological Survey in the United States, continues his valuable contributions to scientific knowledge by the publication of his annual reports; and the volume which is now before us forms part of the Report for 1881-82.

This volume contains 144 pages, besides a full index, and thirty-two lithographic plates. It is carefully and modestly written, and the author candidly admits that our knowledge of the subject treated by him is "very imperfect." The title of the work may be open to a slight criticism; and the word "inland" ("*binnen*" in German) might be preferable to the negative expression "non-marine," which is used by the author.

The geological formations which are embraced in the "Review" are the Devonian, Carboniferous, Jurassic, Triassic, Cretaceous, Laramie, and Tertiary. With respect to the Laramie formation, the author regards the group as occupying a transitional position between the Cretaceous and Tertiary; it is remarkably fossiliferous, inasmuch as a greater number of the species mentioned in the "Review" come from that group than from any other. The total number of North American non-marine or inland fossil species and well-marked varieties appears to be 227, of which 141 are found in the Laramie formation or group. Twelve species are Palæozoic, and of these no fewer than seven species belong to the Pulmonibranchiata, and to the families *Limacidæ* and *Helicidæ*, which are not only terrestrial mollusca, but undoubtedly air-breathers. *Strophites grandæva* of Dawson, from the Devonian formation, is by far the most ancient land shell hitherto known to us. In the face of these facts and in the absence of any facts to warrant the conclusion of the author, how can we reasonably agree with him "that molluscan life began in the sea, and that all fresh-water and land mollusca have been primarily derived from those of marine origin"? Although no land mollusk has yet been discovered in the oldest fossiliferous formation, it is evident that land and consequently terrestrial conditions must then and long previously have existed, so as to account for the sedimentary strata of which that formation consisted and for the prevalence of *Lingula* and other shallow-water Brachiopoda in the Silurian epoch.

It is curious to notice that so many species of what are usually considered marine Conchifera (*Ostrea*, *Anomia*, and *Mytilus*) occur in the Laramie group, and one of *Anomia* in the Cretaceous formation. This confirms the experiments of Beudant and other naturalists, that many marine gill-bearing mollusks can live either in their own native and proper element or can gradually become accustomed to a brackish and ultimately a fresh-water habitat. The author also notes the "persistence through long periods of geological time of even the simpler types of non-marine mollusks, after they were once established." And he remarks with respect to the Gastropoda that, "although in geological rank the Gasteropoda are so much in advance of the Conchifera, the various families of the former seem to have been developed as early in geological time as those of the latter, and so far as we are now acquainted with the history of the fossil non-marine mollusca of North America, it appears that highly-organised land pulmonate Gasteropods were introduced quite as early as any of the Conchifers. Indeed from present indications we are led to believe that the relations of the different classes of non-marine mollusca to each other were much the same in all geological epochs as they are to-day." The following is also interesting:—"Notwithstanding the annual migration of myriads of aquatic birds between the northern and southern provinces of North America at the present time, and doubtless also ever since it has been a continent, the fresh-water molluscan fauna of those regions respectively are still distinct."

A few minor points of classification which are met with in the present work will not be accepted by conchologists without some hesitation, *e.g.* the extension of the so-called family *Rissoidæ* (or more properly *Littorinidæ*) so as to include the genera *Hydrobia* and *Bythinella*, which latter is a subgenus of *Bythinia* and belongs to the *Paludinidæ*. The families *Pisidiidæ*, *Physidæ*, *Ancylidæ*, *Vitrinidæ*, *Arionidæ*, *Pupidæ*, *Succinidæ*, and *Viviparidæ* seem to be also superfluous. In every well-organised army there ought to be a due proportion of men to officers of different ranks.

J. GWYN JEFFREYS

OUR BOOK SHELF

Plant-Life. By Edward Step. Third Edition. (London: T. Fisher Unwin, 1884.)

THIS is another attempt to give a popular description of some of the more sensational parts of the science of botany; though the plan is disconnected, the general idea of the book would not be bad, provided it were well carried out. It is to be regretted that the author has failed to realise that it is necessary to be accurate in popular description. For instance, it is gravely stated in italics that roots are never green (p. 29); we also read that *Ruscus aculeatus* "presents the remarkable appearance of a flower growing in the centre of a leaf" (p. 94), that the Cryptogams have no embryo (p. 211), and that the *Ricciaceæ* and *Characeæ* have stomata (p. 212)! On p. 171 he mistakes intercellular spaces for cells in *Isoëtes*, which he classes under the *Marsiliaceæ* (spelt *Marcileaceæ*, p. 212); and on p. 165 we are informed that the elaters

of *Equisetum* are composed of cells. As is usual in works such as this, the terminology of the reproductive organs of the lower forms is very erratic: thus he uses the terms "spore" and "antheridium" as equivalent in *Selaginella* (p. 139), he calls the "sporogonia" of the Mosses "sporangia," and the "oogonia" of *Fucus* "perispores."

On the subject of the lichen-gonidia theory he waxes warm, stating (p. 150) that it has been "termed sensational romance by every well-known practical fungologist and lichenologist." Of course every one is free to express his own opinion, but few who are not blind partisans will be prepared to agree with Mr. Step in excluding such men as De Bary, Schwendener, and Stahl from the list of "well-known practical fungologists and algologists."

Till so-called popular books are written with more accuracy, we should strongly advise those who wish to dabble in science either to abstain, or, better, to brace their minds to attack some text-book which can be depended upon: after this, if they wish, they can easily supply for themselves that cheap sentiment with which "Plant-Life" abounds.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Equatorial Coudé of the Paris Observatory

IT was only on the 7th of this month that my attention was called to a letter in NATURE (p. 4) from M. Lœwy, headed "Reply to Mr. Grubb's Criticisms on the Equatorial *Coudé* of the Paris Observatory." I deferred answering this letter as I was travelling at the time, and had not the necessary data by me to refer to; further, because I desired to see the second part of M. Lœwy's letter before making my reply; and lastly, because I hoped that some of the errors in M. Lœwy's first letter were misprints, and that he would have corrected them in his second.

I may observe in passing that the paper to which M. Lœwy alludes is a description of an instrument in the construction of which I am at present engaged, and that any criticisms of the equatorial *coudé* which it contained were merely incidental.

M. Lœwy assumes that my instrument is a modification of one which he described in 1871 in the *Comptes Rendus*. This description I have, as it happens, never seen, nor am I much concerned to defend myself from the suggestion of plagiarism, since the fundamental principle of what I have called the "siderostatic telescope" is one the advantages of which were recognised long before 1871, although mechanical difficulties prevented its application.

I find some difficulty in rendering my reply intelligible to your readers, for although the equatorial *coudé* has been described and figured in NATURE, they are not as yet familiar with the form of the instrument which I am at present constructing. I now proceed to consider M. Lœwy's first letter.

I cannot admit that M. Lœwy's recital of what he is pleased to call my criticisms is accurate. He says:—

"To give weight to his argument Mr. Grubb examines a case of the construction of an instrument of 27 inches aperture, and he anticipates in the construction the following difficulties which he considers insurmountable:—(1) The optical difficulty of constructing a large plane mirror. (2) The practical difficulty of procuring a disk of the necessary dimensions. Mr. Grubb affirms there is no glass-works capable of making a disk of glass so large. (3) The difficulty of moving a mirror of which the weight, according to Mr. Grubb's calculations, will be very nearly half a ton. (4) The dearness of the instrument, which would cost more than an ordinary equatorial plus dome and observatory."

With regard to (1) I may remind M. Lœwy that I distinctly admitted the "*possibility*" of constructing such a mirror, but I expressed the opinion that inasmuch as no mirror of this size had yet been attempted success was problematical.

I now learn from M. Lœwy that such a mirror has been made, and that it "leaves absolutely nothing to be desired"; but he has not told us to what tests this mirror has been subjected. I am not aware that there exists in Paris an object-glass of sufficient size to embrace the whole pencil of light reflected from it, and without this it is not possible, in my opinion, to apply tests which can be legitimately considered final.

(2) I did say that *I believed* it to be impossible to obtain disks of such large dimensions, and I founded my belief on the fact that not many years ago I applied to several of the principal glass manufacturers of England, France, and Germany for prices of disks for mirrors, the weight of which would have been less than those mentioned by M. Lœwy, and none were willing to undertake the work. I am glad, however, to learn from M. Lœwy that the glass manufacturers are now prepared to furnish disks of such a size.

(3) The next difficulty M. Lœwy attributes to me is "the difficulty of moving a mirror of which the weight, according to Mr. Grubb's calculations, will be very nearly half a ton," and this, he says, "*has arisen from some error in calculation.*"

I would ask M. Lœwy to point out where I said anything of the difficulty of moving a mirror of half a ton weight. As I had a considerable share in the construction of the Melbourne mirrors, which weighed nearly four times this, it is hardly likely that I would assert such an absurdity; and as to the allegation of a wrong calculation, I ask M. Lœwy to point out the mistake he refers to, and in order to give him every facility for so doing I append it below. It is necessary first, however, to settle the matter of thickness.

M. Lœwy, in NATURE, May 1 (p. 5), says:—"I have never said that the thickness should be 0·18 of the diameter. I have given 0·18 as a *maximum*."

I have, however, before me a paper presented by M. Lœwy to the Academy of Sciences, March 19, 1883, in which occurs the following passage:—"Mes recherches m'ont démontré que pour prévenir dans un miroir toute déformation causée par la flexion ou un léger *serrage*, il faut que l'épaisseur du verre soit de 0·18 du diamètre." Now if I understand this rightly, it means that he has found 0·18 of the diameter to be the *minimum* thickness necessary, and in no part of that paper does he speak of 0·18 as a *maximum*. He mentions in the following paragraph that a thickness of one-fourth is necessary under certain conditions; and further on in the same paper he says:—"C'est dans ces conditions que les miroirs du nouvel instrument ont été construits." This being so, I still think I was justified in assuming 0·225, a mean between one-fifth and one-fourth, as that which he recommended. Now as to diameter. He assumes a diameter of 38 inches to be sufficient for a 27-inch objective, but $27 \times \sqrt{2} = 38{\cdot}178$. Even this would only give full aperture of objective for the central pencil. For a moderate field nearly 38½ would be in actual use.

Again, no optician in practice thinks of making a mirror without some margin; in small sizes one-fourth to one-half of an inch, but in such a size as this three-fourths of an inch all round is not an extravagant allowance, and therefore in adopting 40 inches diameter I did not exaggerate.

I therefore had as my data—

Diameter 40 inches.
Thickness $40 \times 0{\cdot}225 = 9$ inches.

The calculation then is simple.

$$\text{Solid contents in cubic feet} = \frac{\text{Area of 40 inch circle} \times 9}{1728} = 6{\cdot}547 \text{ cubic feet};$$

and, taking a specific gravity of 2·5, each cubic foot would weigh 156¼ lbs.; therefore the weight of mirror = $6{\cdot}547 \times 156{\cdot}25 =$ 1023 lbs., or *rather more than* 9 *cwt.* I said nearly half a ton.

I may add that the specific gravity of the glass supplied to me by Messrs. Chance is rather more, viz. 2·52, which would give a higher result.

In another place M. Lœwy represents me as finding that "the mirror necessary for an equatorial *coudé* of 27 inches would weigh 8½ tons"!!

I never made such a statement, and I challenge M. Lœwy to say what grounds he has for this assertion.

This is the second case in which he attributes to me statements which I have not made. A third and a fourth we shall come to just now. Your readers will remark that M. Lœwy (except in one minor instance) never quotes my words. Hence perhaps have arisen the serious mistakes into which he has fallen. Respecting the matter of thickness, M. Lœwy appears to have